

## ARTICLE

# Seasonal agricultural activity and crime

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**Abstract**

Sudden shocks to labor demand have sometimes been shown to increase local crime rates. We build on this literature by estimating the causal effect of labor-intensive seasonal agricultural activity on crime. We analyze a unique data set that describes criminal activity and fruit, vegetable, and horticultural (FVH) employment by month and U.S. county from 1990 to 2016. We find that the FVH labor share is associated with reduced property and violent crime rates, and possibly the number of property crimes committed within county years. Examining heterogeneities based on ethnicity, labor-intensive FVH activity decreases the rate of non-Hispanic arrests and victimization, and increases the number of Hispanic arrests and victims (consistent with rising local Hispanic populations). Taken together, results are broadly consistent with the idea that agricultural harvest of labor-intensive crops enhances local labor market opportunities that reduce incentives to commit crimes. Results are robust to a battery of alternative specifications that address the inherent challenges associated with measuring seasonal agricultural labor.

**KEYWORDS**

crime, farm workers, immigration, seasonal agriculture, seasonal employment

**JEL CLASSIFICATION**

E24, F22, K14, R23, Q10

## 1 | INTRODUCTION

Fruit, vegetable, and horticultural (FVH) production is characterized by large seasonal changes in labor demand, much of which is met by migrant laborers. An estimated 38% of seasonal FVH workers were classified as migratory from 1990 to 2016, and approximately 48% were unauthorized

immigrants over the same span.<sup>1</sup> Many Americans believe that immigrants, and especially “illegal” immigrants, are more likely to commit violent crimes than the rest of the US population.<sup>2</sup> Such sentiments—along with anecdotal evidence—can lead to fear that agricultural activity causes crime. For example, Huron, California, a quintessential Central Valley town populated by farm workers, has been called “knife-fight city” in reference to the ubiquity of knives used to harvest head lettuce during the spring combined with high poverty and crime (Martin et al., 2006). Further to the west, when agricultural guest worker housing units were constructed in Spreckels, California, local residents raised concerns that the presence of seasonal farm workers would increase crime and subsequently reduce home values (Mohan, 2017). Nevertheless, rigorous investigation is required to test whether these fears and beliefs can be validated by a causal relationship between seasonal farm activities and crime. In this paper, we identify the short-term effects of seasonal, labor-intensive FVH activity on crime using monthly sector-specific employment data and crime reporting data at the county level from 1990 to 2016.

The relationship between crime and agricultural activity is theoretically ambiguous. On the one hand, migrant farm workers are predominantly male, and tend to be poor and relatively uneducated—all of which is associated with increased criminal activity (Campaniello & Gavrilova, 2018; Kelly, 2000; Lochner & Moretti, 2004).<sup>3</sup> On the other hand, there is empirical evidence that immigrants are equally (Bell et al., 2013; Bianchi et al., 2012; Chalfin, 2014; Reid et al., 2005) or less (Baker, 2015; Butcher & Piehl, 2007; Stowell et al., 2009; Wadsworth, 2010) likely to commit crimes than natural-born citizens, and an estimated 78% of FVH workers in the United States were foreign born between 1990–2016.<sup>4,5</sup> To the extent that agricultural activity creates economic spillovers that enhance local labor market opportunities (which we do find evidence of), this could also reduce incentives to commit crimes (Blakeslee & Fishman, 2018; Carr & Packham, 2019; Foley, 2011; Freedman et al., 2018; Freedman & Owens, 2016; Gould et al., 2002; Lin, 2008; Watson et al., 2019).<sup>6</sup> However, in certain instances significant and sudden increases in economic activity have also been shown to increase crime, as with the U.S. shale energy boom (Gourley & Madonia, 2018; James & Smith, 2017; Komarek, 2018; Street, 2018), a result James and Smith (2017) hypothesize could be explained by migration and subsequent “social disorganization” in which the sudden inflow and outflow of people disrupts social cohesion, making it easier to successfully commit crimes without being caught (Freudenburg, 1986; Sampson & Groves, 1989).<sup>7</sup> It is also possible that migration increases crime rates among non-immigrant populations. For example, immigration has been shown to increase crime rates among non-migrant residents who face increased employment competition (Borjas et al., 2010). Furthermore, migrant farm workers often lack (perceived or actual) legal protections, thus potentially contributing to their frequent victimization (Moyce & Schenker, 2018; Wallis, 2019).

<sup>1</sup>Based on authors’ analysis of the National Agricultural Workers Survey (NAWS). The NAWS defines a worker as migratory if they reported jobs that were at least 75 miles apart or who reported moving more than 75 miles to obtain a farm job during a 12-month period.

<sup>2</sup>Data collected from a 2018 Grinnell College National Poll that asked 1000 U.S. adults, “Compared to the U.S. population overall, do you think the rate of violent crime committed by illegal immigrants in the United States is higher, lower, or about the same?” Although 30% of respondents answered “higher” just 20% answered “lower.” Detailed results are available at: <https://www.pollingreport.com/immigration.htm>. This idea is reinforced by earlier survey data from 2000 in which 73.4% of respondents thought that it was “very likely” or “somewhat likely” that crime rates would increase as a result of increased immigration into the United States (Spenkuch, 2014).

<sup>3</sup>71.1% of FVH workers in 2016 who reported working the previous year had an annual income below \$25,000, and 33.4% had income below \$15,000. In the online supplementary Appendix S1, we summarize statistics on income distribution, labor migration, weeks not working, and other variables of potential interest for FVH workers in the National Agricultural Workers Survey (NAWS).

<sup>4</sup>Based on authors’ analysis of the Department of Labor, Employment, and Training Administration (2017) for fiscal year 2016.

<sup>5</sup>One notable exception is (Spenkuch, 2014) who finds that increasing the U.S. county migrant share of the population is associated with more burglaries, larcenies, and grand theft auto (but no effect on violent crimes). He further finds that these effects only hold for immigrants from Mexico, who he posits have relatively poor labor market opportunities and so might be prone to commit financially motivated crimes.

<sup>6</sup>Whether agricultural activity creates meaningful short run economic spillovers remains an open question. However, the literature finds little to no evidence of long-run economic spillovers from the agricultural sector to other local non-farm sectors (Foster & Rosenzweig, 2004; Hornbeck & Keskin, 2015; Weber et al., 2015)

<sup>7</sup>Whereas well acquainted neighbors with established friendships are more likely to keep a watchful eye on their neighbor’s house, they may be less likely to recognize an ongoing burglary at a stranger’s house. Relatedly, committing a crime in an environment of strong social organization is especially risky as witnesses are more likely to recognize the perpetrator’s face and identify them to police.

Despite these theoretical ambiguities and the attention this topic has received in popular media outlets, to our knowledge we are the first to examine how seasonal labor-intensive agricultural activity impacts local crime rates.<sup>8</sup> We fill this gap by combining Uniform Crime Reporting (UCR) data on crime counts and seasonal agricultural employment at the county-by-month level over the period 1990–2016. We analyze how seasonal variation in FVH employment is associated with seasonal patterns of crime. Causal inference is facilitated by the granularity of our data. Although there may be unobserved factors that are correlated with agricultural activity and crime (such as population density or income), such factors tend to be specific to particular counties or years. But by exploiting county-by-month data, all identifying variation comes from within the county-year level. Our key identifying assumption is that within county-year variation in unobserved determinants of criminal activity is uncorrelated with variation in our measure of labor-intensive agricultural activity after controlling for month-by-year fixed effects. As long as this assumption holds, we are able to estimate the short run (e.g., within year) causal effect of agricultural activity in FVH sectors on crime, but this comes at the cost of foregoing analysis of longer run, more permanent effects that might also be important.

Our analysis is made up of two parts. First, we estimate the marginal effect of an increase in the seasonal agricultural employment share of the labor force on measures of property and violent crime. Understanding marginal effects is important from a policy perspective, but this baseline analysis potentially masks important non-linearities in the relationship between seasonal employment and criminal activity. To address this, we supplement our baseline specification with a semi-parametric one that describes how crime rates change each month relative to the month when FVH-intensive counties have their peak seasonal farm workforce.

We find that a one percentage point increase in the seasonal FVH employment share is associated with roughly five fewer property crimes per 100,000 members of the labor force<sup>9</sup> (compared to a sample average of 388 property crimes per 100,000). Consonant results are found for the violent crime rate, though with inconsistent statistical significance. Our semi-parametric specification reinforces these findings. Relative to five months before peak seasonal employment, property and violent crime rates are roughly 12% lower during the peak seasonal employment month in treated counties (defined as those with significant seasonal FVH employment shares) relative to control counties.

We also investigate potential mechanisms driving these negative crime effects. As discussed above, two possibilities are that migrant workers are less likely to commit crimes than the non-migrant population and that the non-migrant population may commit fewer crimes during seasonal FVH activity due to economic spillovers. To explore these possibilities we conduct an analysis of National Incident-Based Reporting System (NIBRS) data, which identify (in some cases) the ethnicity of both the victims and arrestees associated with a crime (though the NIBRS program has much lower participation rates from police precincts than the UCR program). A number of important insights emerge. First, during labor-intensive FVH seasons, both the victimization and arrest rate fall among non-Hispanics. Because the majority of seasonal agricultural workers are Hispanic, this suggests that the crime rate falls for the non-migrant population during labor-intensive seasons, consistent with the hypothesis that seasonal agricultural activity broadly improves local economic conditions and reduces the incentive to commit crimes. Second, although we do not find effects on Hispanic arrest rates, we find that the Hispanic victimization rate rises in response to seasonal labor-intensive FVH activity. In this case, the corresponding offenders are largely Hispanic, though there is evidence that some of these offenders are also non-Hispanic or of unknown ethnicity.

To further investigate mechanisms, we examine whether crime is affected by corn harvesting season in corn-intensive counties rather than FVH-intensive counties. We choose corn because it is

<sup>8</sup>To our knowledge, the most closely related paper to ours is Blakeslee and Fishman (2018), which estimates effects of weather-driven agricultural income shocks on crime in India. Unlike the present study, Blakeslee and Fishman (2018) analyze this relationship in a developing country and do not study seasonal crime effects.

<sup>9</sup>Because population data are not available at the county-by-month level, we proxy crime rates with the number of crimes divided by the labor force. See Section 2.1 for further discussion.

widely grown but is not nearly as labor intensive to harvest. We do not find evidence of crime effects in this case, suggesting that the crime reductions we find for FVH are driven specifically by labor market impacts rather than overall income effects (assuming that corn harvests provide a contemporaneous increase in income in corn-producing counties).

This study contributes most directly to the literature examining the social and economic effects of agricultural activity. To our knowledge we are the first to analyze how seasonal patterns in labor-intensive agricultural activity are associated with seasonal patterns in crime. We also contribute more generally to the literature examining the effects of labor demand shocks and immigration on local crime rates. As mentioned above, this literature includes analysis of the American shale energy boom, which is associated with increased crime (James & Smith, 2017), and also studies of various international immigration shocks, which predominately find a negative association with crime, though results vary.

## 2 | DATA

### 2.1 | Employment data

Our employment data come from the Quarterly Census of Employment and Wages (QCEW), a census of all establishments that are covered by unemployment insurance compiled by the Bureau of Labor Statistics (BLS). The QCEW provides month-by-county-by-industry employment counts for all counties and years from 1975 to the present. Industries are classified by NAICS codes, and employment counts are available at the six-digit level.<sup>10</sup> One pitfall of these data is that when there are few employers in a given county-industry-year combination (or some other reason that employers could be identifiable), wage and employment data are suppressed. When employment data for any of our seasonal agricultural sectors are suppressed we will under measure the seasonal employment share. In the online supplementary Appendix S1, we discuss this issue further and we provide a robustness check in which we drop observations with suppressed seasonal agricultural sectors.

To the extent that employers do not report unauthorized workers for unemployment insurance, we may also under count seasonal farm workers in the QCEW. However, since the Immigration Reform and Control Act (IRCA) was passed in 1986, employers are held legally responsible for knowingly hiring unauthorized workers (Taylor & Charlton, 2018). Thus, farm employers have incentive to require workers to provide some form of legal documentation. Many unauthorized workers provide a false social security number to their employers and would therefore be counted in the QCEW.<sup>11</sup>

Another concern with the QCEW is that farm employers with few employees are not required to report workers for unemployment insurance in some states, and consequently, farm employers with few employees in these states do not record their workers in the QCEW. Furthermore, employers in some states are required to report H-2A agricultural guest workers for unemployment insurance, whereas employers in other states are not.<sup>12</sup> According to Rural Migration News (2020), farm employers of all sizes in Washington and California must report all employees for unemployment insurance, including H-2A workers, but farm employers in Florida do not. Florida, North Carolina, Georgia, Washington, and California employed half of all H-2A workers in 2016 (Martin, 2017).<sup>13</sup>

<sup>10</sup>We drop any county-year observations that report zero total employment for any month within the year, though this is very rare.

<sup>11</sup>It is estimated that undocumented immigrants contributed \$13 billion to Social Security funds and \$3 billion to Medicare in 2016 by submitting false social security numbers to employers for payroll (Roberts, 2019).

<sup>12</sup>H-2A is a non-immigrant guest worker visa for seasonal farm workers. Program take-up was extremely low between 1986–2010, but rose rapidly from 2011 to 2018. Nevertheless, in 2016 H-2A workers made up only 7% of the national farm workforce (Martin, 2017).

<sup>13</sup>According to conversations with several of the leading researchers in the field of farm labor economics, including administrators of the NAWS, there is no known database indicating which states have a threshold number of employees below which agricultural employers do not report to Unemployment Insurance. According to a phone call with the North Carolina Department of Commerce, farm employers in North Carolina do not report H-2A workers in the QCEW or any farm employees if total employees is fewer than 10 employees in 20 weeks of a calendar year or payroll less than \$20,000 per year. The Georgia Department of Labor was unable to disclose any information about what Georgia employers do or do not report.

This may cause us to under count the seasonal farm work force in key states, particularly if crew leaders for farm labor contractors (FLCs) are considered individual employers. However, we drop Florida from our analysis due to irregular crime data (see Section 2.2), and we know that two of the other leading states in H-2A employment, California and Washington, report H-2A workers in the QCEW along with other farm workers in the same sector. We address the concern that QCEW under counts seasonal farm workers by repeating our analysis using only counties in California and Washington where we know that all employees must be reported in the QCEW.

Additionally, because H-2A agricultural guest workers could differ from other seasonal farm workers given that they have legal temporary guest visas and are subject to the corresponding regulations, we repeat our analysis using H-2A guest worker shares as the explanatory variable.<sup>14</sup> The results from these robustness checks are reported in the online supplementary Appendix S1.

An important caveat for this study is that, although we have county-by-month data on crime counts and employment, we do not have monthly estimates of population, which creates a challenge in estimating rates of crime (as opposed to counts). If the harvest-season employment spike draws workers who work seasonally and remain in the same county even after their employment ends, the increase in employment will exceed the true proportional increase in population. For this reason we calculate monthly crime rates as the number of crimes per total labor force, which is distinct from total employment in that it includes people who are not employed but are looking for work, and so is less sensitive to economic swings and more representative of the working-age population, though the caveat remains that seasonal employment spikes could draw in workers who are otherwise out of the labor force altogether. We draw county-by-month labor force counts from the Bureau of Labor Statistics, which constructs labor force estimates based on several sources, including the Current Population Survey, American Community Survey, the Current Employment Statistics Survey, and state unemployment insurance data. In addition, we run a robustness check that attempts to account for seasonal farm workers who remain in the same county while they are not working. Using data from the National Agricultural Workers Survey (NAWS), we find the percentage of seasonal farm workers who report that they are settled in one location and the annual average share of the year that these workers report that they did not work, and we adjust our labor force denominator accordingly. This exercise is further discussed and presented in the online supplementary Appendix S1. Further, although measurement error in population is of consequence in the interpretation of our estimated effects of seasonal farm labor shares on crime rates, it is not of concern for our analysis of crime counts.

## 2.2 | Crime data

Crime data are drawn from Uniform Crime Reporting (UCR), which is a compilation of incident counts by over 16,000 law enforcement agencies. We use the “Offenses Known and Clearances by Arrest,” which contains counts of reported crimes at the month-by-agency level for several types of offenses. It is important to note that the UCR data are restricted to serious crimes. This is not to say, however, that an abundance of less serious criminal activity is not also important. Our main outcomes of interest are rates of all crimes, violent crimes, and property crimes. Violent crimes include homicide, rape, robbery, and aggravated assault. Property crimes include burglary, larceny, and motor vehicle theft. We aggregate agency-level crime counts to the county level for our analysis.

One key issue with UCR data is that agencies are not required to report crimes. However the data do indicate the number of months reported for a given agency and year. We drop any agency-year combination with less than 12 months of reporting. Therefore we ensure that the jurisdictional populations are equivalent for each month within a county year. The month-level design of this study

<sup>14</sup>Data on the number of H-2A workers per county-month come from the Office of Foreign Labor Certification (OFLC) Disclosure data. Most observations include the worksite county and state for each H-2A application. However, county names were sometimes misspelled or employers reported the city in place of the county. Marcelo Castillo (USDA, Economic Research Service [ERS]) generously shared with us the data that he matched to work site county using data matching methods across employers in multiple years.

makes the UCR reporting problem much less problematic than designs that aggregate to the county-year level because agencies can be added or removed from a county or experience large changes in reporting on a year-to-year basis. Because all of our regressions include county-by-year fixed effects, all identifying variation is within the county-year level where these issues do not apply. Further, some counties are not included at all in the UCR, and this can vary by year. In our main sample, an average of 2587 counties are included per year. Missing counties are typically low in population.<sup>15</sup>

One remaining issue with UCR data is that in some cases even an agency that indicates 12 months of reporting loads a disproportionate number of crimes on a single month. Most commonly in this case, agencies will have zero counts for all months except December, but it sometimes happens for other months as well. To address this, we first drop all counties in Florida and Alabama from our analysis because this issue is extremely common in those states. For remaining counties, within each year we find the month with the highest number of crimes. If the ratio of crimes in this month to the average of all other months within the year is greater than 10, we drop that county-year combination from the analysis (we perform this step separately for violent and property crimes). This step drops less than 1% of observations. The threshold ratio of 10 is meant to remove especially extreme outliers that could skew results. Our methods of dropping counties with misreported crimes data should only introduce sample selection bias if selection is correlated with both seasonal farm labor share and seasonal patterns of crime, which seems unlikely.

Table 1 provides basic summary statistics for the crime and employment data used in our analyses. This table also provides statistics for seasonal employment count and seasonal employment share for the peak seasonal employment month for the treatment group used in our semiparametric specification discussed in the next section.

The table shows means of each variable for the baseline property and violent crime rate regression samples. Standard errors are shown in parenthesis and sample size used in the main regression specifications are in brackets.

### 3 | METHODOLOGY

Production of fruit, vegetable, and horticultural (FVH) crops is characterized by high seasonal variation in labor demand.<sup>16</sup> Low-skilled Mexican immigrants, who make up a large share of seasonal farm workers, respond strongly to geographic variation in labor demand Cadena and Kovak (2016), so locations that experience an especially strong harvest are likely to draw a large number of immigrant workers. We exploit this feature of the farm-worker labor market and estimate the relationship between the seasonal employment share and criminal activity using two different specifications. The first specification (which we call our baseline parametric specification) measures the marginal effect of variation in the seasonal farm labor share on county-month level crime rates. Our second specification (which we call our semi-parametric specification) estimates how crime varies each month relative to the month when agricultural-intensive counties have their peak seasonal farm workforce.

#### 3.1 | Baseline parametric specification

We estimate the marginal impact of seasonal agricultural labor on crime outcomes with the following equation:

<sup>15</sup>Missing counties are fairly evenly spread throughout the country geographically (aside from dropping all counties in Florida and Alabama, as discussed below). Because rural counties are more likely to be missing, the most rural states like Mississippi, Montana, and South Dakota have the highest share of missing counties.

<sup>16</sup>According to the 2012 Agricultural Census, there were 2.7 million workers hired on farms. There were 3 million workers reported in the 2002 Agricultural Census, 2.6 million in 2007, and 2.4 million in 2017. Data come from the U.S. Department of Agriculture, National Agricultural Statistics Service (NASS) <https://quickstats.nass.usda.gov>. Retrieved on April 27, 2020.



TABLE 1 Summary statistics

Variable	
Property crimes per 100,000 labor force	388.3 (285.7) [838,332]
Violent crimes per 100,000 labor force	171.6 (152.1) [814,020]
Labor force	48,969 (158,387) [838,332]
Seasonal employment	57.01 (670.9) [838,332]
Seasonal employment share (pp)	0.08 (0.68) [838,332]
Has non-zero seasonal employment indicator	0.15 (0.36) [838,332]
Seasonal employment in peak month (T group only)	2109 (4306) [14,964]
Seasonal employment share in peak month (T group only)	0.07 (0.07) [14,964]

Note: The table shows means of each variable for the baseline property and violent crime rate regression samples. Standard errors are shown in parenthesis and sample size used in the main regression specifications are in brackets.

$$Y_{imy} = \alpha + \beta * Seasonal\_Share_{imy} + \mu_{my} + \gamma_{iy} + \epsilon_{imy}, \quad (1)$$

where  $Y_{imy}$  is the outcome of interest for county  $i$  in month  $m$  of year  $y$ ,  $Seasonal\_Share_{imy}$  is the share of the labor force taken up by seasonal agricultural laborers (defined below), measured in percentage points. Month-by-year fixed effects are given by  $\mu_{my}$ , and  $\gamma_{iy}$  is county-by-year fixed effects. Month-by-year fixed effects control for any nation-wide month-specific shocks in crime. County-by-year effects control for any factors constant over a calendar year within a county. Therefore, all identifying variation comes from monthly shifts in seasonal agricultural labor shares within a county year, controlling for any monthly national shocks.  $\beta$  is the coefficient of interest and represents the average change in crimes associated with a one percentage point increase in the seasonal agricultural labor share. Standard errors for all regressions are clustered at the county level.

To measure the seasonal agricultural labor share, we begin by identifying 12 fruit vegetable and horticultural (FVH) sectors in the QCEW data. These sectors, by NAICS title, are apple orchards, grape vineyards, strawberry farming, berry (except strawberry) farming, orange groves, citrus (except orange) groves, other vegetable and melon farming (excluding potatoes), other non-citrus fruit farming, fruit and tree nut combination farming, food crops grown under cover (greenhouse), and

nursery and floriculture production.<sup>17</sup> These sectors consist of crops with high shares of seasonal labor demand.

The QCEW employee counts for the sectors listed above do not include labor hired through farm labor contractors (FLCs), who hire farm workers and contract them to work on individual farms for short-term jobs. FLCs provide a service to reduce labor market frictions when many workers are needed in various locations for short periods. We account for employees of FLCs using multiple methods. In our main specification, we include the employees hired under the NAICS title farm labor contractors and crew leaders in the counties where they are reported. However, given that FLCs may transport workers to different counties to work on multiple farms throughout the year, we perform a robustness check in which we estimate the number of FLC workers contracted in each county based on the share of labor expenditures per county attributed to contract labor in the Agricultural Censuses in 2002, 2007, and 2012.<sup>18</sup>

Of course, each of the 12 FVH sectors contain permanent laborers, in addition to seasonal ones, and workers who may work continuously throughout the year on multiple farms. We estimate the number of seasonal laborers in a given month by performing the following steps for each of the 12 FVH sectors and FLCs: First, for a given set of 12 monthly observations within a county-year, we identify the month with the lowest employment count and assume this count is the number of “permanent” jobs for that county-year group of observations. Then for a given county-month observation, the difference between total employment in the specified sector that month and the permanent employment count is our estimate of the number of seasonal workers in the specified FVH sector. We then sum together seasonal employment from all 12 seasonal sectors and FLCs to yield a total seasonal employment count. Total seasonal employment is then divided by the total labor force to yield the seasonal share in Equation (1).

### 3.2 | Semiparametric specification

Observing that employment in seasonal agricultural sectors typically displays a distinct peak period corresponding to harvest season, we alternatively perform a semiparametric empirical design that estimates how crime is affected over time relative to the peak. To do this we first define a “treatment group” of counties that typically have high shares of seasonal agricultural labor, and then for each of these counties identify a “peak” month where seasonal labor shares are highest.

To define a treatment group, for each set of 12 monthly observations within a county year, we find the month with the highest share of seasonal agricultural employees, as defined above. We then find the average of this yearly maximum seasonal share over all years in the sample (1990–2016). We then include a county in the treatment group if this average maximum share exceeds 4%, which is roughly the 95th percentile among counties that have non-zero seasonal labor.<sup>19</sup> This yields 47 treatment counties, which are shown in red in Figure 1. There is a high concentration of treatment counties in the Central, Salinas, and Imperial Valleys of California, and in the major apple-growing regions of Washington state.<sup>20</sup> This is not surprising because seasonal farm labor demands are particularly high in these regions.<sup>21</sup> We drop counties that are below the 4% threshold but are

<sup>17</sup>Employment on potato farms is reported separately from other vegetables, and potato harvests are generally highly mechanized. Therefore, we excluded potatoes from our analysis. See, for example, Patterson (2015) for a cost and return study for potato production in Idaho.

<sup>18</sup>We linearly interpolate shares of labor contracted through FLCs between 2002–2007 and 2007–2012 to impute FLC shares in years between censuses.

<sup>19</sup>Although the choice of threshold is necessarily arbitrary, results are qualitatively similar when using a threshold of 2% or 6%, though somewhat weaker for the former and stronger for the latter, as expected. These results are available upon request.

<sup>20</sup>See for example, Washington Grown. 2020. “Crops by County.” <http://www.wagrown.com/crops-by-county/> Last visited March 31, 2020.

<sup>21</sup>Seasonal farm labor demand is also high in Florida, but we dropped Florida from the analysis for two primary reasons. The first is that Florida does not report all seasonal farm labor in the QCEW. The second is that Florida does not have consistent records in the UCR crime data.



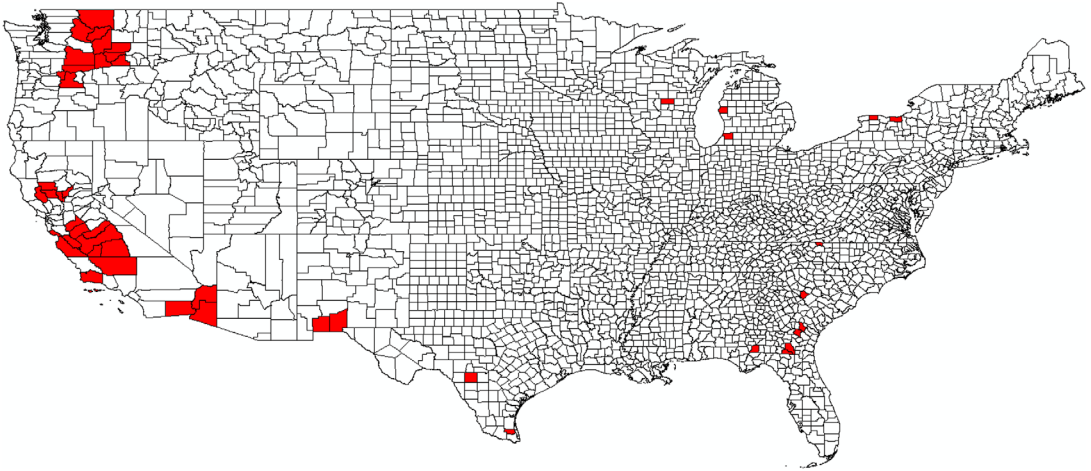


FIGURE 1 Treatment counties

above 1%, as these counties are potentially impacted by seasonal labor, though this does not meaningfully change the results.

For each treated county, we find the peak calendar month for seasonal farm labor, defined as the month with the highest average seasonal labor share across all years in the sample.<sup>22,23</sup> With the treatment group and peak month for each treated county defined, we estimate the following equation:

$$Y_{imy} = \alpha + \sum_{s=-4}^6 \beta_s (\lambda_s T_i) + \mu_{my} + \gamma_{iy} + \epsilon_{imy}, \quad (2)$$

where  $T_i$  is an indicator equal to one if county  $i$  is in the treatment group, and  $\lambda_s$  is an indicator equal to one if the observation is  $s$  months after the peak seasonal labor month. All other variables are defined similarly to Equation (1).  $\beta_s$  then represents the average effect of being  $s$  months after the peak month, where 5 months before the peak month is the omitted category.

We estimate both the parametric and semiparametric specifications to estimate the change in violent and property crime rates, as well as the natural log of crime counts, associated with monthly changes in the seasonal farm workforce. These are complimentary outcomes in evaluating the overall impact on crime. Assuming agricultural activity reduces incentives to commit crimes (by enhancing legal economic opportunities) and that migrants are less likely to commit crimes than other groups, then our estimated effects on crime counts and crime rates will be negative. If agricultural activity does not alter local incentives to commit crimes, and migrants commit crimes at similar rates as non-migrants, then our estimated effects on crime counts will be positive, but effects on crime rates will be zero. If on the other hand, seasonal workers commit crimes at lower but non-zero rates, effects on crime counts will be positive but effects on crime rates negative. Finally, if seasonal workers commit crimes at higher rates or permanent residents commit more crimes in response to increased agricultural activity, then effects on both crime counts and rates will be positive.

<sup>22</sup>The month with the highest share of agricultural labor is not necessarily always the same month within a given county each year. We choose a single calendar month per county to simplify the analysis.

<sup>23</sup>Peak seasonal farm employment months generally run from early summer through the fall. For 44 of our 47 treated counties, the peak month is between May and October, with the most common being July.

**TABLE 2** Property and violent crime results

	<b>A. Property crime results</b>		
	<b>(1)</b> <b>Property crime rate</b>	<b>(2)</b> <b>Ln(Property crime rate)</b>	<b>(3)</b> <b>Ln(Property crime count)</b>
Seasonal emp. share	-4.894 *** (1.277)	-0.012 *** (0.003)	-0.004 (0.003)
<i>N</i>	838,332	814,987	814,987
	<b>B. Violent crime results</b>		
	<b>(1)</b> <b>Violent crime rate</b>	<b>(2)</b> <b>Ln(Violent crime rate)</b>	<b>(3)</b> <b>Ln(Violent crime count)</b>
Seasonal emp. share	-0.207 (0.407)	-0.004 * (0.002)	0.004 *** (0.001)
<i>N</i>	814,020	765,667	765,667

Note: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990–2016. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. \* \*\* \* represent significance at 10%, 5% and 1%, respectively.

Importantly, we should not expect crime count effects to be negative unless non-migrant residents commit fewer crimes in response to increased agricultural activity.

## 4 | RESULTS

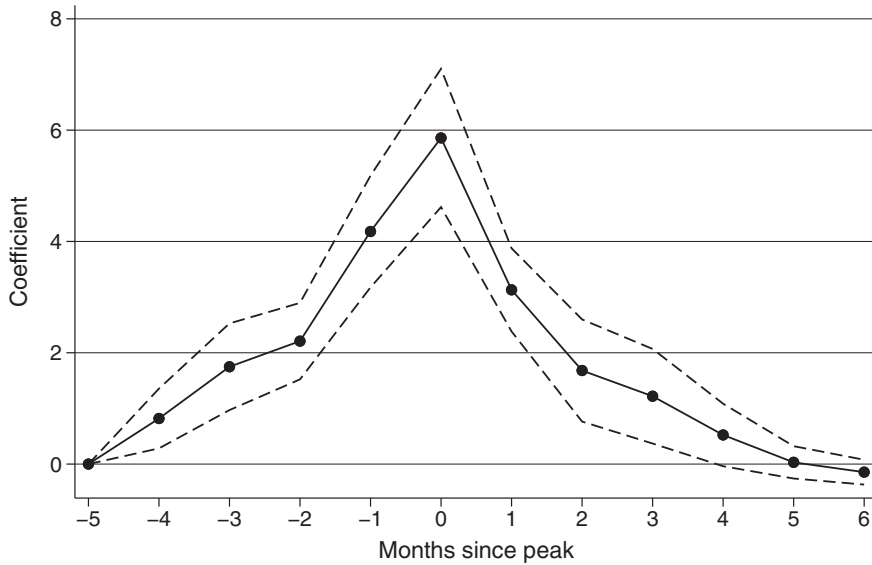
We present the findings from our primary specifications in the sections that follow. First, we present the results from the parametric specification and then the results from the semiparametric specification.

### 4.1 | Baseline parametric specification results

Panel A of Table 2 presents the results from estimating Equation (1) for the property crime rate per 100,000 labor force participants, log of property crime rate, and log of property crime count. Seasonal agricultural labor share is associated with a statistically significant decrease in the property crime rate. The coefficient of  $-4.89$  implies that increasing the seasonal agricultural employment share of labor force by one additional percentage point is associated with 4.89 fewer property crimes per 100,000 labor force participants. This reduction is roughly 1.5% of the sample median property crime rate of 335 per 100,000. We also find a statistically significant reduction on log property crime rates, implying a one percentage point increase in seasonal employment share is associated with a reduction in property crime rates of roughly 1%. Somewhat surprisingly given the influx of temporary laborers, we do not find evidence of effects on property crime counts, and the point estimate is in fact negative. In sum, we find that the increased seasonal labor force share is not associated with an increase in the number of property crimes, and therefore the property crime rate declines as the size of the labor force increases.

Panel B of Table 2 shows the estimated effects on violent crimes. For non-transformed violent crime rates, we find a negative and insignificant effect, though for log violent crime rates the negative effect is significant at a 10% level.<sup>24</sup> Unlike for property crimes, here we do find a positive and

<sup>24</sup>Note that the sample sizes for the log-transformed rate regressions are smaller due to observations with zero crimes, but using this reduced sample for the non-transformed rate regressions does not meaningfully change the result.



**FIGURE 2** Seasonal agricultural employment share, semi-parametric results

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990–2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with seasonal employment share as the dependent variable

significant increase in the count of violent crimes. The estimate of 0.004 implies that a one percentage point increase in seasonal agricultural labor share is associated with a 0.5% increase in violent crime counts. The results for violent crime counts and rates are not contradictory; they collectively imply that the number of violent crimes tends to increase with the influx of seasonal farm labor, but the increase in the labor force is sufficiently large that the measured crime rate falls.

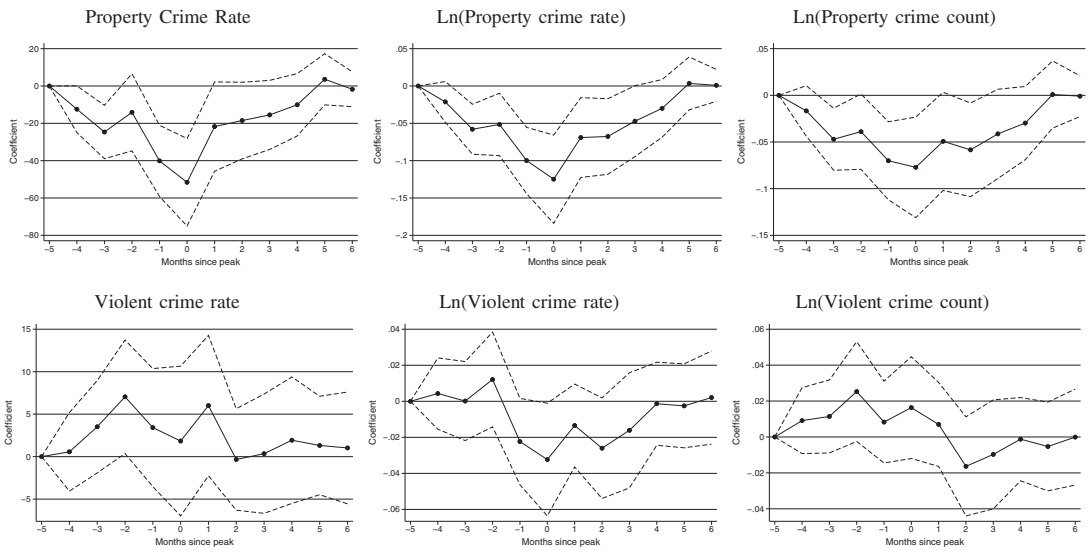
## 4.2 | Semiparametric results

Before presenting the results of the semiparametric specification [Equation (2)], we first demonstrate that our definition of seasonal labor described in Section 1 indeed produces a distinct spike in observed seasonal farm labor in our treatment counties. Figure 2 shows the estimated coefficients from Equation (2) using seasonal agricultural labor share as the dependent variable. The results imply that the seasonal agricultural employment share is on average six percentage points higher relative to control counties during the peak month (or zero “months since peak”) than this same difference 5 months before (the reference category).

Figure 3 shows our semiparametric results for property and violent crimes. They are largely consistent with the parametric results shown in Table 2. For property crimes, the effects on crime rate and log crime rate both experience a dip in the peak month. The negative coefficient estimates are statistically significant at the 5% confidence level for the peak month and one month before.

For log of property crime counts, effects are generally negative relative to five months before peak and intermittently statistically significant, though the dip is much less pronounced. Although we interpret this as merely suggestive and inconclusive evidence for reductions in crime counts, it is an interesting and unexpected result. A speculative interpretation of this finding is that a seasonal rise in agricultural activity improves local economic conditions and reduces the incentives to commit financially motivated crimes. We explore this idea further in Section 4.3 below.

Effects on violent crime rates are largely insignificant, though there is a positive and statistically significant estimate for two months before peak. Similar to the results for property crimes, the log of the



**FIGURE 3** Property and violent crime, semiparametric results

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990–2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2). The dependent variables are indicated in the figure headers. Standard errors are clustered by county

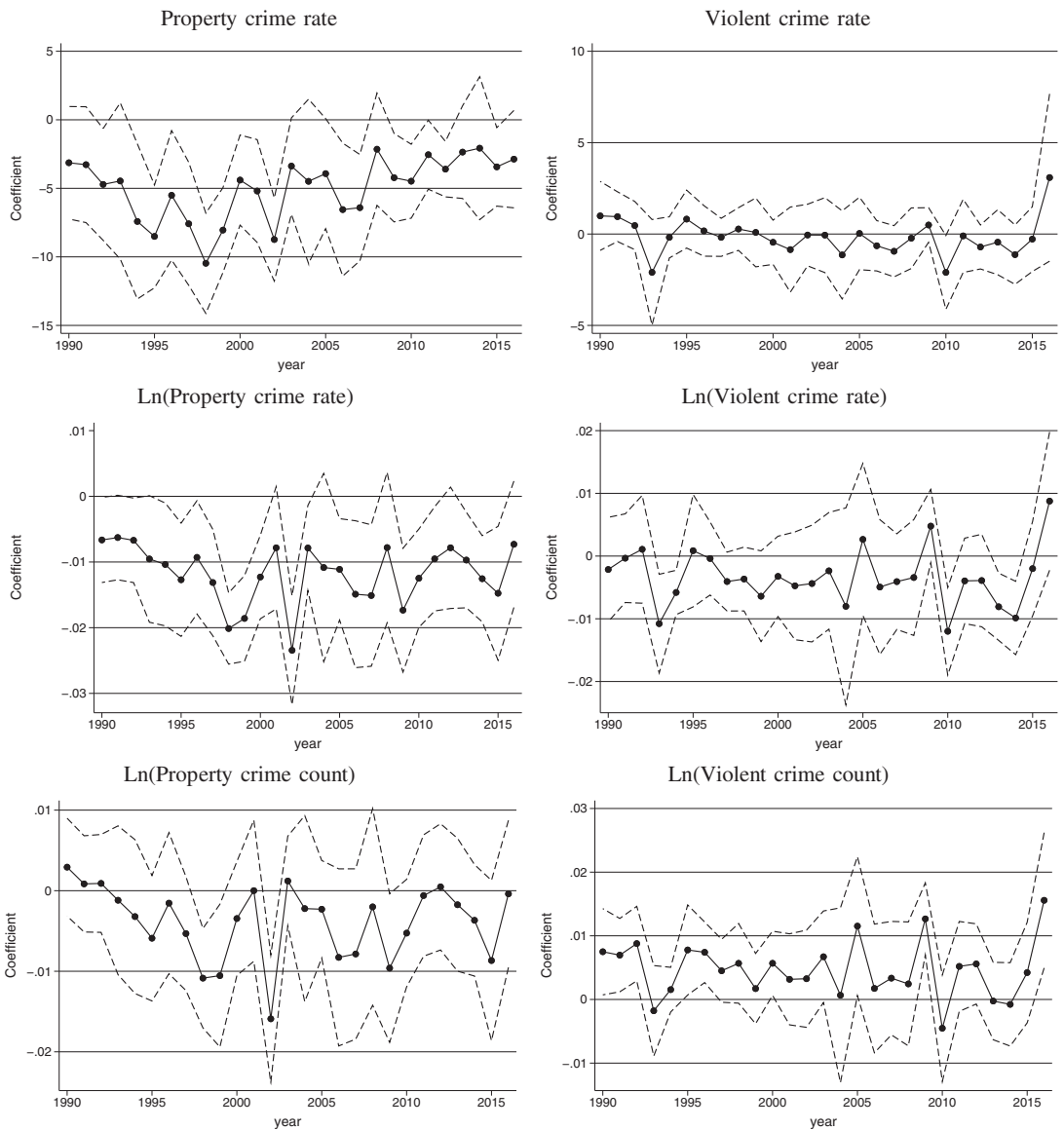
violent crime rate shows a significant dip corresponding to the peak month. The difference in results when using log of violent crime rate could indicate that counties with more seasonal agricultural labor tend to have lower violent crime rates overall and experience large percentage drops in violent crime rates during labor-intensive seasons. For log violent crime counts, there are no statistically significant effects. This is somewhat in contrast to the results using the parametric specification, which showed a small, statistically significant increase in the log violent crime count associated with increased seasonal farm employment. Overall, the semiparametric results are consistent with the parametric results in Table 2, with the exception of finding no statistically significant effects on violent crime counts.

### 4.2.1 | Extension: Year-by-year estimates

Because the identifying variation in our baseline specification shown in Equation (1) is within-year variation, we can identify marginal effects separately for each year, and test whether effects change from 1990–2016. This may be consequential because migration of farm workers declined significantly over this time, due in part to demographic changes in the farm workforce (Fan et al., 2015). We do this for each of our six main outcomes in Figure 4. Estimates are generally consistent with the overall effects shown in Table 2 and fairly trendless throughout the sample period. One exception is that property crime effects are trending down in the first six years of the sample (and non-transformed property crime rate effects gently trend up thereafter, though remain negative throughout). Also, violent crime effects experience a large positive spike in 2016, the last year of the sample.

### 4.3 | Mechanisms

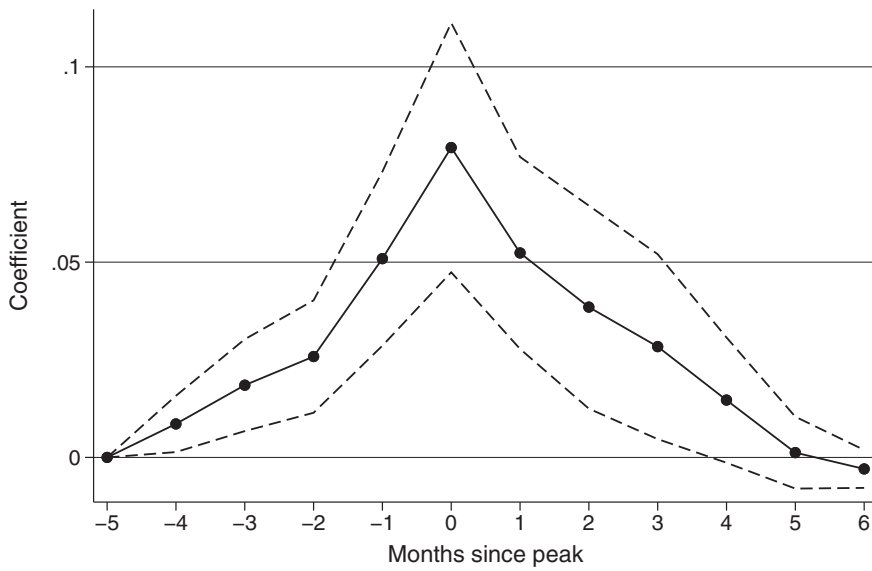
As discussed in the Introduction, there are at least two reasons to think that seasonal agricultural activity reduces crime. First, if increased agricultural activity creates non-agricultural sectoral spillovers and improves labor market opportunities, this possibly raises the opportunity cost of engaging



**FIGURE 4** Crime effects by year

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW). The graphs plot coefficients and 95% confidence intervals from estimating Equation (1) separately for each year from 1990–2016. The dependent variables are indicated in the figure headers below each panel. Standard errors are clustered by county

in illegal activity and reduces crime. Second, according to some estimates, immigrants are *less* likely to commit crimes than non-immigrants (Baker, 2015; Butcher & Piehl, 2007; Stowell et al., 2009; Wadsworth, 2010), and the large majority of migrant farm workers are immigrants. But these effects may be partially offset by the effects of social disorganization and the inward migration of people possibly prone to committing crimes (as discussed earlier). In addition, non-migrant residents may commit crimes against immigrants either because they are perceived to lack any legal power or as means of retribution or because they view migrants as threats to their security or well-being and react preemptively. Although explicitly testing the viability of each of these theories is beyond the scope of this paper, here we explore whether these proposed mechanisms are consistent with available data.



**FIGURE 5** Non-agricultural employment

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990–2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with the natural log of non-agricultural employment as the dependent variable. Standard errors are clustered by county

### 4.3.1 | Economic spillovers

First, we investigate whether seasonal agricultural activity creates short-term economic spillovers into non-farm sectors that could reduce the propensity of permanent residents to commit crimes. There are two necessary conditions that must hold for this “economic” channel to explain our core set of results. The first is that broad labor market conditions improve as the seasonal farm-labor share increases. The second is that the non-migrant crime rate falls in response to seasonal agricultural activity.<sup>25</sup> To test the first condition, we re-estimate Equation (2) with total non-agricultural employment as the dependent variable. The results are consistent with the idea that seasonal agricultural activity generates local economic spillovers. Specifically, Figure 5 shows that non-agricultural employment peaks in tandem with peak seasonal agricultural employment. The fact that non-agricultural employment follows the same inverted “V” pattern as the seasonal employment share could be indicative of increased population driving service sector employment, upstream or downstream linkages to the agricultural sector, or both.

### 4.3.2 | Ethnicity of arrestees and victims

The second necessary condition for the economic channel to hold is that seasonal agricultural activity reduces the non-migrant crime rate. Although we do not observe a non-migrant crime rate, we can use crime data that includes information on ethnicity to shed light on this mechanism. Because 95.3% of migrant FVH workers are Hispanic according to NAWS, the non-Hispanic crime rate can serve as a rough proxy for the crime rate of a subset of the population that is non-migrant (only a subset because of course there are non-migrant Hispanics as well). To do this, we use data from the National Incident-

<sup>25</sup>The extant literature offers an abundance of evidence that economic improvement reduces the incentive to commit property crimes (Baker, 2015; Carr & Packham, 2019; Freedman et al., 2018; Lin, 2008; Watson et al., 2019), but it has little to say about the local economic effects of seasonal agricultural activity.



Based Reporting System (NIBRS), which provides far more detail on individual crimes than UCR, including the ethnicity (if known) of arrestees and victims. However, because precinct participation is voluntary and imposes a heavier reporting burden, it has much lower coverage than UCR. As of 2016, the last year of our data, precincts representing roughly 31% of the US population participated in NIBRS. We use data going back to 2000, as coverage gets especially sparse going further back (about 15% of the population is covered in 2000). These results should therefore be viewed with caution but do provide interesting suggestive evidence regarding the mechanisms of our negative crime effects.

We identify the count of crimes with at least one Hispanic arrestee and the count with at least one non-Hispanic arrestee at the county-month level. We also identify the count of crimes with at least one Hispanic victim, and the count with at least one non-Hispanic victim. We then run our main specification with these counts or corresponding crime rates as the dependent variable.<sup>26,27</sup>

For completeness, we measure crimes in four ways: (1) crime rate (crimes per labor force), (2) the crime count, (3) the natural log of the crime count, and (4) the inverse hyperbolic sine of the crime count to account for observations with zero crimes.<sup>28</sup> We find that, although increasing the seasonal employment share has no effect on aggregate crimes (see the first row of Table 3), it causes a significant reduction in both the number and rate of crimes committed by non-Hispanics. We also find that the seasonal employment share is negatively associated with the rate of non-Hispanic victimization. Given that such a small fraction of migrant farm workers are non-Hispanic, this is consistent with the idea that the positive economic spillovers documented above reduce the incentive to engage in illegal activity.

In contrast to the results for non-Hispanics, increasing the seasonal employment share increases the number of Hispanic victims but not the rate of Hispanic victimization. Although this may simply reflect an increase in the local Hispanic population, it is worth noting that we do not document a corresponding increase in the aggregate number of Hispanic arrests. This is important given that a one percentage point increase in the seasonal employment share amounts to roughly 437 additional workers. Even if migrant farm workers committed crimes at a relatively low rate, one might expect to find the number of crimes committed by this group increases during picking season. These results are consistent with the idea that migrant farm workers commit very few crimes such that, even when their population swells, no new crimes can be detected.

We do however find that the number of Hispanic victims increases in response to an increase in the seasonal employment share of the labor force. In the last three panels of Table 3, we analyze crimes that have at least one Hispanic victim and at least one arrestee that is Hispanic, non-Hispanic, or of unknown ethnicity. The effects on crimes against Hispanics appear to be primarily driven by Hispanic offenders. However, depending on how crime counts are measured, there is evidence that some of these crimes are committed by non-Hispanics and people of unknown ethnicity as well. Taken together, these data are consistent with the idea that agricultural activity both creates economic opportunities that reduce crime and attracts a migrant population that is less prone to criminal activity than other groups, at least during the labor-intensive season of agricultural production. The data are also consistent with the idea that migrant farm workers are victimized by both Hispanic and non-Hispanic people.

### 4.3.3 | Effects of corn harvests

To further disentangle the economic and migration mechanisms, we analyze crime effects of labor shares in corn farming. We choose corn because it is a crop that is grown extensively in the United

<sup>26</sup>In this context, a crime rate is measured as the number of crimes committed by people of Hispanic (or non-Hispanic) ethnicity, relative to the labor force.

<sup>27</sup>As with the UCR data, we drop all precincts in a given calendar year that do not have full reporting for the year. Any county-year combination with no fully reporting precincts are not included in this analysis.

<sup>28</sup>Zero-crime observations are much more common in this analysis than when using UCR data for a few reasons. First, we only have data for ethnicity of an arrestee, and roughly half of crimes in NIBRS do not result in an arrest. Second, the ethnicity of the arrestee/victim is often not known or otherwise not given. Third, many parts of the country have low Hispanic population.

**TABLE 3** Effects by ethnicity of reported offender and victim

	(1) Crime rate	(2) Crime count	(3) Ln(Crime count)	(4) IHS(Crime count)
<b>All crimes</b>				
Seasonal emp. share	-1.962 (3.114)	0.677 (1.156)	0.002 (0.003)	0.002 (0.003)
N	251,018	251,018	246,646	251,018
<b>Non-Hispanic arrests</b>				
Seasonal emp. share	-1.523 *** (0.358)	-0.227 *** (0.068)	-0.005 ** (0.002)	-0.005 ** (0.002)
N	251,018	251,018	208,520	251,018
<b>Hispanic arrests</b>				
Seasonal emp. share	-0.061 (0.236)	0.023 (0.059)	0.006 (0.004)	0.007 * (0.004)
N	251,018	251,018	101,998	251,018
<b>Non-Hispanic victim</b>				
Seasonal emp. share	-4.967 *** (1.441)	-0.921 * (0.490)	0.003 (0.003)	0.002 (0.003)
N	251,018	251,018	215,258	251,018
<b>Hispanic victim</b>				
Seasonal emp. share	0.121 (0.278)	0.161 ** (0.076)	0.009 * (0.005)	0.013 ** (0.005)
N	251,018	251,018	109,997	251,018
<b>Unknown off   Hisp victim</b>				
Seasonal emp. share	0.034 (0.210)	0.090 (0.073)	0.005 (0.004)	0.010 ** (0.005)
N	246,646	246,646	99,506	246,646
<b>Non Hisp off   Hisp victim</b>				
Seasonal emp. share	-0.018 (0.036)	0.009 (0.014)	0.008 *** (0.002)	0.003 (0.003)
N	246,646	246,646	50,623	246,646
<b>Hisp off   Hisp victim</b>				
Seasonal emp. share	0.113 (0.087)	0.065 * (0.035)	0.009 ** (0.004)	0.012 *** (0.004)
N	246,646	246,646	51,257	246,646

Note: based on data from the National Incident-Based Reporting System (NIBRS) from 2000 to 2016. The type of crime for the dependent variable is given in the panel title, and the specific transformation of the type of crime is given in the column header. The "All Crimes" includes crimes for which no arrest is made, whereas all other panels are based only on crimes that include an arrest. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. \* \*\* \* represent significance at 10%, 5% and 1%, respectively.

States, has a common harvest season typically in the fall, but is not nearly as labor-intensive as the harvest of FVH crops and is not grown in large quantity in counties with high FVH employment. Thus, we should not expect corn production activities to attract many migrant workers or correlate with the timing and location of labor-intensive FVH activities. Corn harvest therefore provides variation in farm revenue while holding (roughly) constant the migrant farm worker labor share (which

**TABLE 4** Crimes results: corn employment shares

	Crime rate	Ln(crime rate)	Ln(crime count)
Property crimes			
Seasonal corn emp. share	-2.665 (2.377)	-0.006 (0.012)	-0.005 (0.012)
<i>N</i>	838,332	814,987	814,987
Violent crimes			
Seasonal corn emp. share	-1.564 (1.027)	-0.019 (0.015)	-0.019 (0.015)
<i>N</i>	814,020	765,667	765,667

*Note:* Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990–2016. The table reports estimates when using seasonal corn employment labor force shares as the explanatory variable of interest. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. \* \*\* \*\*\* represent significance at 10%, 5% and 1%, respectively.

does increase during harvest season but at about one tenth of the rate as for FVH sectors). If increased agricultural revenue translates to broad economic improvements and enhanced local labor market opportunities, we expect to find a negative crime effect of corn farming. However, it is also possible that economic spillovers originate from farm worker income, and in this case we should not expect to find significant crime effects of corn farming. The results are shown in Table 4. The estimates are all negative but statistically insignificantly different from zero. This suggests that the economic effects of seasonal agricultural activity on reduced crime are specifically associated with the enhanced employment effects of labor-intensive crops rather than seasonal increases in farm revenues alone.

## 5 | DISCUSSION

It is interesting to compare our estimates to those from analyses of energy booms. James and Smith (2017) find that the shale energy boom increased rates of property crimes as well as some violent crimes (aggravated assault) by roughly 10%–20%. Consonant results are documented by Gourley and Madonia (2018) and Komarek (2018) in their analyses of the Colorado and Pennsylvania shale booms, respectively. Why do our results differ from these related estimates? The answer may be that energy booms appear to attract, rather than create, criminal activity. In fact, Street (2018) finds that the North Dakota oil boom decreased the crime rate among residents that lived in the state prior to the boom (consistent with the idea that improved economic conditions raise the opportunity cost of committing crimes), whereas James and Smith (2017) find that the shale boom had especially large (positive) effects on aggregate criminal activity in North Dakota. Considered jointly, the observed rise in criminal activity in boom towns appears to reflect the inward migration of criminally prone individuals. Our results are consistent with previous findings that show that economic activity can reduce criminal activity among non-migrant residents (Street, 2018) and the literature that finds that foreign-born immigrants are no more likely than natural-born citizens to commit crimes on average (Baker, 2015; Bell et al., 2013; Bianchi et al., 2012; Butcher & Piehl, 2007; Chalfin, 2014; Reid et al., 2005; Stowell et al., 2009; Wadsworth, 2010).

## 6 | ROBUSTNESS

We perform several robustness checks and present the results in the online supplementary Appendix S1. Appendix S1 includes complete explanations and discussions of each robustness check, but we briefly summarize them below.

First, we adjust the labor force denominator to account for seasonal farm workers who might remain in the local region but drop out of the labor force. We use data from the NAWS to estimate the percentage of seasonal farmworkers who are settled in a single location and the mean share of weeks each year that they do not work in any labor sector, and we adjust the labor force denominator accordingly. Results are qualitatively similar to the main results. Second, one might be concerned that employees of FLCs travel to distant farms and do not necessarily work in the county of their employer's address. Thus, we compute the number of FLC workers in each county using the county share of state expenses for contract labor in the Agricultural Census and multiply this share by the total number of FLC workers in the state each month. We use this alternative measure of FLC workers to compute total number of seasonal workers for each observation. Third, we limit the sample to counties in California and Washington states because all farm workers, including H-2A are reported in Unemployment Insurance, and we examine the separate effects of H-2A agricultural guest worker employment shares on crime rates in the national sample. Fourth, we repeat our analysis when dropping counties with any suppressed FVH employment data. Fifth, we repeat our analysis using the inverse hyperbolic sine transformation in place of the natural log. Sixth, we repeat our analysis dropping observations from the election year 2016. Seventh, we repeat our analysis weighting for the total labor force using weighted least squares. Eighth, we repeat our semi-parametric specification using only counties that rank among the top 5% for average employment in corn production as the control group. Our baseline conclusions prove to be quite robust.

Next, in the Appendix S1 we investigate whether Hispanics are more or less likely than non-Hispanics to report being victim to different types of crimes according to the National Crime Victimization Survey. Our findings show that Hispanics who were victim to violent crimes (or attempts) were 1.2 percentage points more likely to report the crime than non-Hispanic victims. However, Hispanics who were victims to personal thefts were 4.1 percentage points less likely to report the crime than non-Hispanic victims. Both of these differences are statistically significant, though qualitatively quite small, and we find no statistically significant difference in the probability of reporting burglary or motor vehicle theft. If seasonal farm workers are more or less likely to report crimes than other residents, this could be problematic for causal identification in our analysis. Nevertheless, the findings from the NIBRS analysis in Table 3 show that seasonal farm labor is associated with a significant decrease in *non-Hispanic* crime victimization. Because few non-Hispanics are seasonal farmworkers, this result cannot be explained by possible differences in the probability that farmworkers report crimes, thus validating the credibility of our main findings even though there is a possibility of relatively small measurement error. Nevertheless, we note that crimes committed against farmworkers, who may have significant obstacles to press charges against offenders<sup>29</sup> are of utmost importance, even if these crimes mostly go undetected by the communities who host seasonal farmworkers and cannot be quantified in available data.

## 7 | CONCLUSION

We estimate the effect of labor-intensive seasonal agricultural activity on crime, and to the best of our knowledge, we are the first to do so. Our analysis is motivated, in part, by the observation that many Americans think immigrants—and undocumented immigrants in particular—are more likely to commit crimes than natural-born citizens, and that many Americans associate seasonal farm labor with drudgery<sup>30</sup> and crime more generally.

We observe both criminal and labor-intensive agricultural activity—measured as the seasonal FVH labor share—by month and U.S. county. The richness of our data allows us to leverage seasonal variation in agricultural activity while controlling for any unobserved factors that are fixed within a county in a given year. We find that increased seasonal FVH labor employment reduces the property crime rate and has little or no effect on the violent crime rate. We also find that it reduces both the rate of crime and of

<sup>29</sup>See for example Soriano (2020).

<sup>30</sup>See for example, Friendly et al., 1960; Steinbeck, 1939.

victimization among the non-Hispanic population (our proxy for the non-migrant farm worker population). Taken together, our results are consistent with idea that agricultural activity improves local economic conditions, which we provide evidence of, and that this reduces the incentive to commit property crimes (Carr & Packham, 2019; Freedman & Owens, 2016; Lin, 2008; Watson et al., 2019). Contrary to potential fears, we find that labor intensive agricultural activity is not associated with increased violent or property crimes and, thus, concerns to the contrary are largely unwarranted.

One caveat to our analyses and conclusion is that we do not know the extent to which crimes committed against migrant farm workers go unreported. As such, it is possible that seasonal agricultural activity is associated with crimes that are undetected in our data. For example, there is anecdotal evidence suggesting that female farm workers are often victims of sexual assault, and the problem may be worse for migrant female workers (Soriano, 2020). We hope that our work will help motivate others to explore this issue further.

## ACKNOWLEDGMENTS

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## SUPPORTING INFORMATION

Additional supporting information may be found in the online version of the article at the publisher's website.

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